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ADDRESS

TO THE

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

1866

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ADDRESS

TO THE

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

DELIVERED BY THE PRESIDENT,

W. R. GROVE, Esq., Q.C., M.A., F.R.S.

AT

Nottingham, August 22, 1866.

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ADDRESS

OF

WILLIAM ROBERT GROVE, ESQ.

PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, NOTTINGHAM, 1866.

IF our rude predecessors, who at one time inhabited the caverns which surround this town, could rise from their graves and see it in its present state, it may be doubtful whether they would have sufficient knowledge to be surprised.

The machinery, almost resembling organic beings in delicacy of structure, by which are fabricated products of world-wide reputation, the powers of matter applied to give motion to that machinery, are so far removed from what must have been the conceptions of the semi-barbarians to whom I have alluded, that they could not look on them with intelligent wonder.

Yet this immense progress has all been effected step by step, now and then a little more rapidly

than at other times; but, viewing the whole course of improvement, it has been gradual, though moving in an accelerated ratio. But it is not merely in those branches of natural knowledge which tend to improvements in economical arts and manufactures, that science has made great progress. In the study of our own planet and the organic beings with which it is crowded, and in so much of the universe as vision, aided by the telescope, has brought within the area of observation, the present century has surpassed any antecedent period of equal duration.

It would be difficult to trace out all the causes which have led to the increase of observational and experimental knowledge.

Among the more thinking portion of mankind the gratification felt by the discovery of new truths, the expansion of faculties, and extension of the boundaries of knowledge, have been doubtless a sufficient inducement to the study of nature; while, to more practical minds, the reality, the certainty, and the progressive character of the acquisitions of natural science, and the enormously increased means which its applications give, have impressed its importance as a minister to daily wants and a contributor to ever-increasing material comforts, luxury, and power.

Though by no means the only one, yet an important cause of the rapid advance of science is the

growth of associations for promoting the progress either of physical knowledge generally, or of special branches of it. Since the foundation of the Royal Society, now more than two centuries ago, a vast number of kindred societies have sprung up in this country and in Europe. The advantages conferred by these societies are manifold; they enable those who are devoted to scientific research, to combine, compare, and check their observations, to assist, by the thoughts of several minds, the promotion of the inquiry undertaken; they contribute from a joint purse to such efforts as their members deem most worthy; they afford a means of submitting to a competent tribunal notices and memoirs, and of obtaining for their authors and others, by means of the discussions which ensue, information given by those best informed on the particular subject; they enable the author to judge whether it is worth his while to pursue the subjects he has brought forward, and they defray the expense of printing and publishing such researches as are thought meritorious.

These advantages, and others might be named, pertain to the Association the thirty-sixth meeting of which we are this evening assembled to inaugurate; but it has, from its intermittent and peripatetic character, advantages which belong to none of the societies which are fixed as to their locality.

Among these are the novelty and freshness of an

annual meeting, which, while it brings together old members of the Association, many of whom only meet on this occasion, always adds a quota of new members, infusing new blood, and varying the social character of our meetings.

The visits of distinguished foreigners, whom we have previously known by reputation, is one of the most delightful and improving of the results. The wide field of inquiry, and the character of communications made to the Association, including all branches of natural knowledge, and varying from simple notices of an interesting observation or experiment, to the most intricate and refined branches of scientific research, is another valuable characteristic.

Lastly, perhaps the greatest advantage resulting from the annual visits of this great parliament to new localities is that, while it imparts fresh local knowledge to the visitors, it leaves behind stimulating memories, which rouse into permanent activity dormant or timid minds—an effect which, so far from ceasing with the visit of the Association, frequently begins when that visit terminates.

Every votary of physical science must be anxious to see it recognised by those institutions of his country which can to the greatest degree promote its cultivation and reap from it the greatest benefit. You will probably agree with me that the principal educational establishments on the one hand, and on

the other the Government, in many of its departments, are the institutions which may best fulfil these conditions. The more early the mind is trained to a pursuit of any kind, the deeper and more permanent are the impressions received, and the more service can be rendered by the students.

> 'Quo semel est imbuta recens, servabit odorem
> Testa diu.'

Little can be achieved in scientific research without an acquaintance with it in youth ; you will rarely find an instance of a man who has attained any eminence in science who has not commenced its study at a very early period of life. Nothing, again, can tend more to the promotion of science than the exertions of those who have early acquired the ἦθος resulting from a scientific education. I desire to make no complaint of the tardiness with which science has been received at our public schools, and, with some exceptions, at our universities. These great establishments have their roots in historical periods, and long time and patient endeavour is requisite before a new branch of thought can be grafted with success on a stem to which it is exotic. Nor should I ever wish to see the study of languages, of history, of all those refined associations which the past has transmitted to us, neglected; but there is room for both. It is sad to see the number of so-called educated men

who, travelling by railway, voyaging by steamboat, consulting the almanac for the time of sunrise or full-moon, have not the most elementary knowledge of a steam-engine, a barometer, or a quadrant; and who will listen with a half-confessed faith to the most idle predictions as to weather or cometic influences, while they are in a state of crass ignorance as to the cause of the trade winds, or the form of a comet's path. May we hope that the slight infiltration of scientific studies, now happily commenced, will extend till it occupies its fair space in the education of the young, and that those who may be able learnedly to discourse on the Eolic digamma will not be ashamed of knowing the principles on which the action of an air-pump, an electrical machine, or a telescope, depends, and will not, as Bacon complained of his contemporaries, despise such knowledge as something mean and mechanical.

To assert that the great departments of Government should encourage physical science may appear a truism, and yet it is but of late that it has been seriously done; now, the habit of consulting men of science on important questions of national interest is becoming a recognised practice, and in a time, which may seem long to individuals, but is short in the history of a nation, a more definite sphere of usefulness for national purposes will, I have no doubt, be provided for those duly qualified men who may be content to give up the more

tempting study of abstract science for that of its practical applications. In this respect the report of the Kew Committee for this year affords a subject of congratulation to those whom I have the honour to address. The Kew Observatory, the petted child of the British Association, may possibly become an important national establishment; and if so, while it will not, I trust, lose its character of a home for untrammelled physical research, it will have superadded some of the functions of the Meteorological Department of the Board of Trade, with a staff of skilful and experienced observers.

This is one of the results which the general growth of science, and the labours of this Association in particular, have produced; but I do not propose on this occasion to recapitulate the special objects attained by the Association; this has been amply done by several of my predecessors; nor shall I confine my address to the progress made in physical science since the time when my most able and esteemed friend and predecessor addressed you at Birmingham. In the various reports and communications which will be read at your sections, details of every step which has been made in science since our last meeting will be brought to your notice, and I have no doubt fully and freely discussed.

I purpose, with your kind permission, to submit to you certain views of what has within a com-

paratively recent period been accomplished by science, what have been the steps leading to the attained results, and what, as far as we may fairly form an opinion, is the general character pervading modern discovery.

It seems to me that the object we have in view would be more nearly approached, by each president, chosen as they are in succession as representing different branches of science, giving on these occasions either an account of the progress of the particular branch of science he has cultivated, when that is not of a very limited and special character, or enouncing his own view of the general progress of science; and though this will necessarily involve much that belongs to recent years, the confining a president to a mere *résumé* of what has taken place since our last meeting would, I venture with diffidence to think, limit his means of usefulness, and render his discourse rather an annual register than an instructive essay.

I need not dwell on the commonplace but yet important topics of the material advantages resulting from the application of science; I will address myself to what, in my humble judgement, are the lessons we have learned, and the probable prospects of improved natural knowledge.

One word will give you the key to what I am about to discourse on; that word is *continuity*, no new word, and used in no new sense, but perhaps

applied more generally than it has hitherto been. We shall see, unless I am much mistaken, that the developement of observational, experimental, and even deductive knowledge is either attained by steps so extremely small as to form really a continuous ascent; or, when distinct results apparently separate from any co-ordinate phenomena have been attained, that then, by the subsequent progress of science, intermediate links have been discovered uniting the apparently segregated instances with other more familiar phenomena. We shall see that the more we investigate, the more we find that in existing phenomena graduation from the like to the seemingly unlike prevails, and in the changes which take place in time, gradual progress is, and apparently must be, the course of nature.

Let me now endeavour to apply this view to the recent progress of some of the more prominent branches of science.

In Astronomy, from the time when the earth was considered a flat plain bounded by a flat ocean —when the sun, moon, and stars were regarded as lanterns to illuminate this plain—each successive discovery has brought with it similitudes and analogies between this earth and many of the objects of the universe, with which our senses, aided by instruments, have made us acquainted. I pass, of course, over those discoveries which have established the Copernican system as applied to our

sun, its attendant planets, and their satellites. The proofs, however, that gravitation is not confined to our solar system, but pervades the universe, have received many confirmations by the labours of members of this Association; I may name those who have held the office of President, Lord Rosse, Lord Wrottesley, and Sir J. Herschel, the two latter having devoted special attention to the orbits of double stars, the former to those probably more recent systems called nebulæ. Double stars seem to be orbs analogous to our own sun and revolving round their common centre of gravity in a conic-section curve, as do the planets with which we are more intimately acquainted; but the nebulæ present more difficulty, and some doubt has been expressed whether gravitation, such as we consider it, acts with those bodies (at least those exhibiting a spiral form) as it does with us; possibly some other modifying influence may exist, our present ignorance of which gives rise to the apparent difficulty. There is, however, another class of observations quite recent in its importance, and which has formed a special subject of contribution to the reports and transactions of this Association; I allude to those on Meteorites, at which our lamented member, and to many of us our valued friend, Prof. Baden Powell, assiduously laboured, for investigations into which a committee of this Association is formed, and a series of star-charts for enabling

observers of shooting-stars to record their observations was laid before the last meeting of the Association by Mr. Glaisher.

It would occupy too much of your time to detail the efforts of Bessel, Schwinke, the late Sir J. Lubbock, and others, as applied to the formation of star-charts for aiding the observation of meteorites which Mr. Alexander Herschel, Mr. Brayley, Mr. Sorby, and others are now studying.

Dr. Olmsted explained the appearance of a point from which the lines of flight of meteors seem to radiate, as being the perspective vanishing point of their parallel or nearly parallel courses appearing to an observer on the earth as they approach it. The uniformity of position of these radiant points, the many corroborative observations on the direction, the distances, and the velocities of these bodies, the circumstance that their paths intersect the earth's orbit at certain definite periods, and the total failure of all other theories which have been advanced, while there is no substantial objection to this, afford evidence almost amounting to proof that these are cosmical bodies moving in the interplanetary space by gravitation round the sun, and some perhaps round planets. This view gives us a new element of continuity. The universe would thus appear not to have the extent of empty space formerly attributed to it, but to be studded between the larger and more visible masses with

smaller planets, if the term be permitted to be applied to meteorites.

Observations are now made at the periods at which meteors appear in greatest numbers—at Greenwich by Mr. Glaisher, at Cambridge by Prof. Adams, and at Hawkhurst by Mr. Alexander Herschel—and every preparation is made to secure as much accuracy as can, in the present state of knowledge, be secured for such observations.

The number of known asteroids, or bodies of a smaller size than what are termed the ancient planets, has been so increased by numerous discoveries, that instead of seven we now count eighty-eight as the number of recognised planets—a field of discovery with which the name of Hind will be ever associated.

The smallest of these is only twenty or thirty miles in diameter, indeed cannot be accurately measured, and if we were to apply the same scrutiny to other parts of the heavens as has been applied to the zone between Mars and Jupiter, it is no far-fetched speculation to suppose that, in addition to asteroids and meteorites, many other bodies exist until the space occupied by our solar system becomes filled up with planetary bodies varying in size from that of Jupiter (1240 times larger in volume than the earth) to that of a cannon-ball or even a pistol-bullet.

The researches of Leverrier on the intra-mer-

curial planets come in aid of these views; and another half-century may, and not improbably will, enable us to ascertain that the now seemingly vacant interplanetary spaces are occupied by smaller bodies which have hitherto escaped observation, just as the asteroids had until the time of Olbers and Piazzi. But the evidence of continuity as pervading the universe does not stop at telescopic observation; chemistry and physical optics bring us new proofs. Those meteoric bodies which have from time to time come so far within reach of the earth's attraction as to fall upon its surface, give on analysis metals and oxides similar to those which belong to the structure of the earth—they come as travellers bringing specimens of minerals from extra-terrestrial regions.

In a series of papers recently communicated to the French Academy, M. Daubrée has discussed the chemical and mineralogical character of meteorites as compared with the rocks of the earth. He finds that the similarity of terrestrial rocks to meteorites increases as we penetrate deeper into the earth's crust, and that some of the deep-seated minerals have a composition and characteristics almost identical with meteorites [olivine, herzolite, and serpentine, for instance, closely resemble them]; that as we approach the surface, rocks having similar components with meteorites are found, but in a state of oxidation, which neces-

sarily much modifies their mineral character, and which, by involving secondary oxygenised compounds, must also change their chemical constitution. By experiments he has succeeded in forming from terrestrial rocks substances very much resembling meteorites. Thus close relationship, though by no means identity, is established between this earth and those wanderers from remote regions, some evidence, though at present incomplete, of a common origin.

Surprise has often been expressed that, while the mean specific gravity of this globe is from five to six times that of water, the mean specific gravity of its crust is barely half as great. It has long seemed to me that there is no ground for wonder here. The exterior of our planet is to a considerable depth oxidated; the interior is in all probability free from oxygen, and whatever bodies exist there are in a reduced or deoxidated state; if so, their specific gravity must necessarily be higher than that of their oxides or chlorides, &c.; we find, moreover, that some of the deep-seated minerals have a higher specific gravity than the average of those on the surface; olivine, for instance, has a specific gravity of 3·3. There is therefore no à priori improbability that the mean specific gravity of the earth should notably exceed that of its surface; and if we go further, and suppose the interior of the earth to be formed of the same

ingredients as the exterior, minus oxygen, chlorine, bromine, &c., a specific gravity of 5 to 6 would not be an unlikely one. Many of the elementary bodies entering largely into the formation of the earth's crust are as light or lighter than water—for instance, potassium, sodium, &c.; others, such as sulphur, silicon, aluminium, have from two to three times its specific gravity; others, again, as iron, copper, zinc, tin, seven to nine times; while others, lead, gold, platinum, &c., are much more dense—but, speaking generally, the more dense are the least numerous. There seems no improbability in a mixture of such substances producing a mean specific gravity of from 5 to 6, although it by no means follows, indeed the probability is rather the other way, that the proportions of the substances in the interior of the earth are the same as on the exterior. It might be worth the labour to ascertain the mean specific gravity of all the known minerals on the earth's surface, averaging them in the ratios in which, as far as our knowledge goes, they quantitatively exist, and assuming them to exist without the oxygen, chlorine, &c., with which they are, with some rare exceptions, invariably combined on the surface of the earth: great assistance to the knowledge of the probable constitution of the earth might be derived from such an investigation.

While chemistry, analytic and synthetic, thus

aids us in ascertaining the relationship of our planet to meteorites, its relation in composition to other planets, to the sun, and to more distant suns and systems, is aided by another science, viz. optics.

That light passing from one transparent medium to another should carry with it evidence of the source from which it emanates, would, until lately, have seemed an extravagant supposition; but probably (could we read it) everything contains in itself a large portion of its own history.

I need not detail to you the discoveries of Kirchhoff, Bunsen, Miller, Huggins, and others; they have been dilated on by my predecessor. Assuming that spectrum analysis is a reliable indication of the presence of given substances by the position of transverse bright lines exhibited when they are burnt, and of transverse dark lines when light is transmitted through their vapours, though Plücker has shown that with some substances these lines vary with temperature, the point of importance in the view I am presenting to you is, that while what may be called comparatively neighbouring cosmical bodies exhibit lines identical with many of those shown by the components of this planet, as we proceed to the more distant appearances of the nebulæ we get but one or two of such lines, and we get one or two new bands not yet identified with any known to be produced by substances on this globe.

Within the last year Mr. Huggins has added to his former researches observations on the spectrum of a comet (comet 1 of 1866), the nucleus of which shows but one bright line, while the spectrum formed by the light of the coma is continuous, seeming to show that the nucleus is gaseous, while the coma would consist of matter in a state of minute division shining by reflected light: whether this be solid, liquid, or gaseous is doubtful, but the author thinks it is in a condition analogous to that of fog or cloud. The position in the spectrum of the bright line furnished by the nucleus is the same as that of nitrogen, which line is also shown in some of the nebulæ.

But the most remarkable achievement by spectrum analysis is the record of observations on a temporary star which has shone forth this year in the constellation of the northern crown about a degree S.E. of the star ε. When it was first seen, May 12, it was nearly equal in brilliancy to a star of the second magnitude; when observed by Mr. Huggins and Dr. Miller, May 16, it was reduced to the third or fourth magnitude. Examined by these observers with the spectroscope, it gave a spectrum which they state was unlike that of any celestial body they had examined.

The light was compound and had emanated from two different sources. One spectrum was analo-

gous to that of the sun, viz. formed by the light of an incandescent solid or liquid photosphere which had suffered absorption by the vapours of an envelope cooler than itself. The second spectrum consisted of a few bright lines, which indicated that the light by which it was formed was emitted by matter in the state of luminous gas. The observers consider that, from the position of two of the bright lines, the gas must be probably hydrogen, and from their brilliancy compared with the light of the photosphere the gas must have been at a very high temperature. They imagine the phenomena to result from the burning of hydrogen with some other element, and that from the resulting temperature the photosphere is heated to incandescence.

There is strong reason to believe that this star is one previously seen by Argelander and Sir J. Herschel, and that it is a variable star of long or irregular period; it is also notable that some of its spectrum lines correspond with those of several variable stars. The time of its appearance was too short for any attempt to ascertain its parallax; it would have been important if it could even have been established that it is not a near neighbour, as the magnitude of such a phenomenon must depend upon its distance. I forbear to add any speculations as to the cause of this most singular phenomenon; however imperfect the knowledge given us by these observations, it is a great triumph to have

caught this fleeting object, and obtained permanent records for the use of future observers.

It would seem as if the phenomenon of gradual change obtained towards the remotest objects with which we are at present acquainted, and that the further we penetrate into space the more unlike to those we are acquainted with become the objects of our examination—sun, planets, meteorites, earth similarly though not identically constituted, stars differing from each other and from our system, and nebulæ more remote in space and differing more in their characters and constitution.

While we can thus to some extent investigate the physical constitution of the most remote visible substances, may we not hope that some further insight as to the constitution of the nearest, viz. our own satellite, may be given us by this class of researches? The question whether the moon possesses any atmosphere may still be regarded as unsolved. If there be any, it must be exceedingly small in quantity and highly attenuated. Calculations, made from occultations of stars, on the apparent differences of the semidiameter of the bright and dark moon give an amount of difference which might indicate a minute atmosphere, but which Mr. Airy attributes to irradiation.

Supposing the moon to be constituted of similar materials to the earth, it must be, to say the least, doubtful whether there is oxygen enough to oxidate

the metals of which she is composed; and if not, the surface which we see must be metallic, or nearly so. The appearance of her craters is not unlike that seen on the surface of some metals, such as bismuth, or, according to Professor Phillips, silver, when cooling from fusion and just previous to solidifying; and it might be a fair subject of enquiry whether, if there be any coating of oxide on the surface, it may not be so thin as not to disguise the form of the congealed metallic masses, as they may have set in cooling from igneous fusion. M. Chacornac's recent observations lead him to suppose that many of the lunar craters were the result of a single explosion, which raised the surface as a bubble and deposited its débris around the orifice of eruption.

The eruptions on the surface of the moon clearly did not take place at one period only, for at many parts of the disk craters may be seen encroaching on and disfiguring more ancient craters, sometimes to the extent of three or four successive displacements: two important questions might, it seems to me, be solved by an attentive examination of such portions of the moon. By observing carefully with the most powerful telescopes the character of the ridges thus successively formed, the successive states of the lunar surface at different epochs might be elucidated; and secondly, as on the earth we should look for actual volcanic action at those points where recent eruptions have taken place,

so on the moon the more recently active points being ascertained by the successive displacement of anterior formations, it is these points which should be examined for existing disruptive disturbances. Metius and Fabricius might be cited as points of this character, having been found by M. Chacornac to present successive displacements and to be perforated by numerous channels or cavities. M. Chacornac considers that the seas, as they are called, or smoother portions of the lunar surface have at some time made inroads on anteriorly formed craters; if so, a large portion of the surface of the moon must have been in a fused, liquid, semiliquid, or alluvial state long after the solidifying of other portions of it. It would be difficult to suppose that this state was one of igneous fusion, for this could hardly exist over a large part of the surface without melting up the remaining parts; on the other hand, the total absence of any signs of water, and of any, or, if any, only the most attenuated, atmosphere, would make it equally difficult to account for a large diluvial formation.

Some substances, like mercury on this planet, might have remained liquid after others had solidified; but the problem is one which needs more examination and study before any positive opinion can be pronounced.

I cannot pass from the subject of lunar physics

without recording the obligation we are under to our late President for his most valuable observations and for his exertion in organising a band of observers devoted to the examination of this our nearest celestial neighbour, and to Mr. Nasmyth and Mr. De la Rue for their important graphical and photographical contributions to this subject. The granular character of the sun's surface observed by Mr. Nasmyth in 1860 is also a discovery which ought not to be passed over in silence.

Before quitting the subject of astronomy I cannot avoid expressing a feeling of disappointment that the achromatic telescope, which has rendered such notable service to this science, still retains in practice the great defect which was known a century ago at the time of Hall and Dollond, namely, the inaccuracy of definition arising from what was termed the irrationality of the spectrum, or the incommensurate divisions of the spectra formed by flint and crown glass.

The beautiful results obtained by Blair have remained inoperative from the circumstance that evaporable liquids being employed between the lenses, a want of permanent uniformity in the instrument was experienced; and notwithstanding the high degree of perfection to which the grinding and polishing object-glasses has been brought by Clarke, Cooke, and Mertz, notwithstanding the

greatly improved instrumental manufacture, the defect to which I have adverted remains unremedied and an eyesore to the observer with the refracting telescope.

We have now a large variety of different kinds of glass formed from different metallic oxides. A list of many such was given by M. Jacquelain a few years back; the last specimen which I have seen is a heavy high refracting glass formed from the metal thallium by M. Lamy. Among all these, could not two or three be selected which, having appropriate refracting and dispersing powers, would have the coloured spaces of their respective spectra if not absolutely in the same proportions, at all events much more nearly so than those of flint and crown glass? Could not, again, oily or resinous substances having much action on the more refrangible rays of the spectrum, such as castor oil, canada balsam, &c., be made use of in combination with glass lenses to reduce if not annihilate this signal defect? This is not a problem to the solution of which there seems any insuperable difficulty; the reason why it has not been solved is, I incline to think, that the great practical opticians have no time at their disposal to devote to long tentative experiments and calculations, and on the other hand the theoretic opticians have not the machinery and the skill in manipulation requisite to give the appropriate degree of

excellence to the materials with which they experiment; yet the result is worth labouring for, as, could the defect be remedied, the refracting telescope would make nearly as great an advance upon its present state as the achromatic did on the single lens refractor.

While gravitation, physical constitution, and chemical analysis by the spectrum show us that matter has similar characteristics in other worlds than our own, when we pass to the consideration of those other attributes of matter which were at one time supposed to be peculiar kinds of matter itself, or, as they were called, imponderables, but which are now generally, if not universally, recognised as forces or modes of motion, we find the evidence of continuity still stronger.

When all that was known of magnetism was that a piece of steel rubbed against a particular mineral had the power of attracting iron, and, if freely suspended, of arranging itself nearly in a line with the earth's meridian, it seemed an exceptional phenomenon. When it was observed that amber, if rubbed, had the temporary power of attracting light bodies, this also seemed something peculiar and anomalous. What are now magnetism and electricity? forces so universal, so apparently connected with matter as to become two of its invariable attributes, and that to speak of matter not being capable of being affected by these forces would seem almost as

extravagant as to speak of matter not being affected by gravitation.

So with light, heat, and chemical affinity, not merely is every form of matter with which we are acquainted capable of manifesting all these modes of force, but so-called matter supposed incapable of such manifestations would to most minds cease to be matter.

Further than this it seems to me (though, as I have taken an active part for many years, now dating from a quarter of a century, in promoting this view, I may not be considered an impartial judge) that it is now proved that all these forces are so invariably connected *inter se* and with motion as to be regarded as modifications of each other, and as resolving themselves objectively into motion, and subjectively into that something which produces or resists motion, and which we call force.

I may perhaps be permitted to recal a forgotten experiment, which nearly a quarter of a century ago I showed at the London Institution, an experiment simple enough in itself, but which then seemed to me important from the consequences to be deduced from it, and the importance of which will be much better appreciated now than then.

A train of multiplying wheels ended with a small metallic wheel which, when the train was put in motion, revolved with extreme rapidity against the

periphery of the next wheel, a wooden one. In the metallic wheel was placed a small piece of phosphorus, and as long as the wheels revolved the phosphorus remained unchanged, but the moment the last wheel was stopped, by moving a small lever attached to it, the phosphorus burst into flame. My object was to show that while motion of the mass continued, heat was not generated, but that when this was arrested, the force continuing to operate, the motion of the mass became heat in the particles. The experiment differed from that of Rumford's cannon-boring and Davy's friction of ice in showing that there was no heat while the motion was unresisted, but that the heat was dependent on the motion being impeded or arrested. We have now become so accustomed to this view, that whenever we find motion resisted we look to heat, electricity, or some other force as the necessary and inevitable result.

It would be out of place here, and treating of matters too familiar to the bulk of my audience, to trace how by the labours of Oersted, Seebeck, Faraday, Talbot, Daguerre, and others, materials have been provided for the generalisation now known as the correlation of forces or conservation of energy, while Davy, Rumford, Seguin, Mayer, Joule, Helmholtz, Thomson, and others (among whom I would not name myself, were it not that I may be misunderstood and supposed to have aban-

doned all claim to a share in the initiation of this, as I believe, important generalisation) have carried on the work; and how, sometimes by independent and, as is commonly the case, nearly simultaneous deductions, sometimes by progressive and accumulated discoveries, the doctrine of the reciprocal interaction, of the quantitative relation, and of the necessary dependence of all the forces has, I think I may venture to say, been established.

If magnetism be, as it is proved to be, connected with the other forces or affections of matter, if electrical currents always produce, as they are proved to do, lines of magnetic force at right angles to their lines of action, magnetism must be cosmical, for where there is heat and light there is electricity, and consequently magnetism. Magnetism, then, must be cosmical and not merely terrestrial. Could we trace magnetism in other planets and suns as a force manifested in axial or meridional lines, *i. e.* in lines cutting at right angles the curves formed by their rotation round an axis, it would be a great step; but it is one hitherto unaccomplished. The apparent coincidences between the maxima and minima of solar spots, and the decennial or undecennial periods of terrestrial magnetic intensity, though only empirical at present, might tend to lead us to a knowledge of the connection we are seeking; and the President of the Royal Society considers that an additional epoch of coin-

cidence has arrived, making the fourth decennial period; but some doubt is thrown upon these coincidences by the magnetic observations made at Greenwich Observatory. In a paper published in the 'Transactions of the Royal Society,' 1863, the Astronomer Royal says, speaking of results extending over seventeen years, there is no appearance of decennial cycle in the recurrence of great magnetic disturbances; and Mr. Glaisher last year, in the physical section of this Association, stated that after persevering examination he had been unable to trace any connection between the magnetism of the earth and the spots on the sun.

Mr. Airy, however, in a more recent paper, suggests that currents of magnetic force having reference to the solar hour are detected, and seem to produce vortices or circular disturbances, and he invites further cooperative observation on the subject, one of the highest interest, but at present remaining in great obscurity.

One of the most startling suggestions as to the consequence resulting from the dynamical theory of heat is that made by Mayer, that by the loss of *vis viva* occasioned by friction of the tidal waves, as well as by their forming, as it were, a drag upon the earth's rotatory movement, the velocity of the earth's rotation must be gradually diminishing, and that thus, unless some undiscovered compensatory action exist, this rotation must ultimately cease,

and changes hardly calculable take place in the solar system.

M. Delaunay considers part of the acceleration of the moon's mean motion which is not at present accounted for by planetary disturbances, to be due to the gradual retardation of the earth's rotation; to which view, after an elaborate investigation, the Astronomer Royal has given his assent.

Another most interesting speculation of Mayer is that with which you are familiar, viz. that the heat of the sun is occasioned by friction or percussion of meteorites falling upon it: there are some difficulties, not perhaps insuperable, in this theory. Supposing such cosmical bodies to exist in sufficient numbers they would, as they revolve round the sun, fall into it, not as an aërolite falls upon the earth directly by an intersection of orbits, but by the gradual reduction in size of the orbits, occasioned by a resisting medium; some portion of force would be lost, and heat generated in space by friction against such medium; when they arrive at the sun they would, assuming them, like the planets, to have revolved in the same direction, all impinge in a definite direction, and we might expect to see some symptoms of such in the sun's photosphere; but though this is in a constant state of motion, and the direction of these movements has been carefully investigated by Mr. Carrington

and others, no such general direction is detected; and M. Faye, who some time ago wrote a paper pointing out many objections to the theory of solar heat being produced by the fall of meteoric bodies into the sun, has recently investigated the proper motions of sun spots, and believes he has removed certain apparent anomalies and reduced their motions to a certain regularity in the motion of the photosphere, attributable to some general action arising from the internal mass of the sun.

It might be expected that comets, bodies so light and so easily deflected from their course, would show some symptoms of being acted on by gravitation, were such a number of bodies to exist in or near their paths as are presupposed in the mechanical theory of solar heat.

Assuming the undulatory theory of light to be true, and that the motion which constitutes light is transmitted across the interplanetary spaces by a highly elastic ether, then unless this motion is confined to one direction, unless there be no interference, unless there be no viscosity, as it is now termed, in the medium, and, consequently, no friction, light must lose something in its progress from distant luminous bodies, that is to say, must lose something as light; for, as all reflecting minds are now convinced that force cannot be annihilated, the force is not lost, but its mode of action is changed. If light, then, is lost as light (and the

observations of Struvé seem to show this to be so, that, in fact, a star may be so far distant that it can never be seen in consequence of its luminous emissions becoming extinct), what becomes of the transmitted force lost as light, but existing in some other form? So with heat: our sun, our earth, and planets are constantly radiating heat into space, so in all probability are the other suns, the stars, and their attendant planets. What becomes of the heat thus radiated into space? If the universe have no limit, and it is difficult to conceive one, heat and light should be everywhere uniform; and yet more is given off than is received by each cosmical body, for otherwise night would be as light and as warm as day. What becomes of the enormous force thus apparently non-recurrent in the same form? Does it return as palpable motion? Does it move or contribute to move suns and planets? And can it be conceived as a force similar to that which Newton speculated on as universally repulsive and capable of being substituted for universal attraction? We are in no position at present to answer such questions as these; but I know of no problem in celestial dynamics more deeply interesting than this, and we may be no further removed from its solution than the predecessors of Newton were from the simple dynamical relation of matter to matter which that potent intellect detected and demonstrated.

Passing from extra terrestrial theories to the narrower field of molecular physics, we find the doctrine of correlation of forces steadily making its way. In the Bakerian Lecture for 1863, Mr. Sorby shows, not perhaps a direct correlation of mechanical and chemical forces, but that when, either by solution or by chemical action, a change in volume of the resulting substance as compared with that of its separate constituents is effected, the action of pressure retards or promotes the change, according as the substance formed would occupy a larger or smaller space than that occupied by its separate constituents; the application of these experiments to geological inquiries as to subterranean changes which may have taken place under great pressure is obvious, and we may expect to form compounds under artificial compression which cannot be found under normal pressure.

In a practical point of view the power of converting one mode of force into another is of the highest importance, and with reference to a subject which at present, somewhat prematurely, perhaps, occupies men's minds, viz. the prospective exhaustion of our coal-fields, there is every encouragement derivable from the knowledge that we can at will produce heat by the expenditure of other forces; but, more than that, we may probably be enabled to absorb or store up, as it were, diffused energy—for instance, Berthelot has found that the potential

energy of formate of potash is much greater than that of its proximate constituents, caustic potash and carbonic oxide. This change may take place spontaneously and at ordinary temperatures, and by such change carbonic oxide becomes, so to speak, reinvested with the amount of potential energy which its carbon possessed before uniting with oxygen, or, in other words, the carbonic oxide is raised as a force-possessor to the place of carbon by the direct absorption or conversion of heat from surrounding matter.

Here we have as to force-absorption, an analogous result to that of the formation of coal from carbonic acid and water; and though this is a mere illustration, and may never become economical on a large scale, still it and similar examples may calm apprehension as to future means of supplying heat, should our present fuel become exhausted. As the sun's force, spent in times long past, is now returned to us from the coal which was formed by that light and heat, so the sun's rays, which are daily wasted, as far as we are concerned, on the sandy deserts of Africa, may hereafter, by chemical or mechanical means, be made to light and warm the habitations of the denizens of colder regions. The tidal wave is, again, a large reservoir of force hitherto almost unused.

The valuable researches of Professor Tyndall on radiant heat, afford many instances of the power of

localising, if the term be permitted, heat which would otherwise be dissipated.

The discoveries of Graham, by which atmospheric air, drawn through films of caoutchouc, leaves behind half its nitrogen, or, in other words, becomes richer by half in oxygen, and hence has a much increased potential energy, not only show a most remarkable instance of physical molecular action, merging into chemical, but afford us indications of means of storing up force, much of the force used in working the aspirator being capable at any period, however remote, of being evolved by burning the oxygen with a combustible.

What changes may take place in our modes of applying force before the coal-fields are exhausted it is impossible to predict. Even guesses at the probable period of their exhaustion are uncertain. There is a tendency to substitute for smelting in metallurgic processes, liquid chemical action, which of course has the effect of saving fuel; and the waste of fuel in ordinary operations is enormous, and can be much economised by already known processes. It is true that we are, at present, far from seeing a practical mode of replacing that granary of force the coal-fields; but we may with confidence rely on invention being in this case, as in others, born of necessity, when the necessity arises.

I will not further pursue this subject; at a time

when science and civilisation cannot prevent large tracts of country being irrigated by human blood in order to gratify the ambition of a few restless men, it seems an over-refined sensibility to occupy ourselves with providing means for our descendants in the tenth generation to warm their dwellings or propel their locomotives.

Two very remarkable applications of the convertibility of force have been recently attained by the experiments of Mr. Wilde and Mr. Holz; the former finds that, by conveying electricity from the coils of a magneto-electric machine to an electro-magnet, a considerable increase of electrical power may be attained, and by applying this as a magneto-electric machine to a second, and this in turn to a third electro-magnetic apparatus, the force is largely augmented. Of course, to produce this increase, more mechanical force must be used at each step to work the magneto-electric machines; but provided this be supplied, there hardly seems a limit to the extent to which mechanical may be converted into electrical force.

Mr. Holz has contrived a Franklinic electrical machine, in which a similar principle is manifested. A varnished glass plate is made to revolve in close proximity to another plate having two or more pieces of card attached, which are electrified by a bit of rubbed glass or ebonite; the moment this is effected, a resistance is felt by the operator who

turns the handle of the machine, and the slight temporary electrization of the card converts into a continuous flood of intense electricity the force supplied by the arm of the operator.

These results offer great promise of extended application; they show that, by a mere formal disposition of matter, one force may be converted into another, and that not to the limited extent hitherto attained, but to an extent co-ordinate, or nearly so, with the increased initial force, so that, by a mere change in the arrangement of apparatus, a means of absorbing and again eliminating in a new form a given force may be obtained to an indefinite extent. As we may, in a not very distant future, need, for the daily uses of mankind, heat, light, and mechanical force, and find our present resources exhausted, the more we can invent new modes of conversion of forces, the more prospect we have of practically supplying such want. It is but a month from this time that the greatest triumph of force-conversion has been attained. The chemical action generated by a little salt-water on a few pieces of zinc will now enable us to converse with inhabitants of the opposite hemisphere of this planet, and

> 'Put a girdle round about the earth in forty minutes.'

The Atlantic Telegraph is an accomplished fact.

In physiology very considerable strides are being

made by studying the relation of organised bodies to external forces; and this branch of enquiry has been promoted by the labours of Carpenter, Bence Jones, Playfair, E. Smith, Frankland, and others. Vegetables acted on by light and heat, decompose water, ammonia, and carbonic acid, and transform them into, among other substances, oxalate of lime, lactic acid, starch, sugar, stearine, urea, and ultimately albumen; while the animal reverses the process, as does vegetable decay, and produces from albumen, urea, stearine, sugar, starch, lactic acid, oxalate of lime, and ultimately ammonia, water, and carbonic acid.

As, moreover, heat and light are absorbed, or converted in forming the synthetic processes going on in the vegetable, so conversely heat and sometimes light is given off by the living animal; but it must not be forgotten that the line of demarcation between a vegetable and an animal is difficult to draw, that there are no single attributes which are peculiar to either, and that it is only by a number of characteristics that either can be defined.

The series of processes above given may be simulated by the chemist in his laboratory; and the amount of labour which a man has undergone in the course of twenty-four hours may be approximately arrived at by an examination of the chemical changes which have taken place in his body; changed forms in matter indicating the anterior exercise of

dynamical force. That muscular action is produced or supported by chemical change would probably now be a generally accepted doctrine; but while many have thought that muscular power is derived from the oxidation of albuminous or nitrogenised substances, several recent researches seem to show that the latter is rather an accompaniment than a cause of the former, and that it is by the oxidation of carbon and hydrogen compounds that muscular force is supplied. Traube has been prominent in advancing this view, and experiments detailed in a paper published this year by two Swiss professors, Drs. Fick and Wislicenus, which were made by and upon themselves in an ascent of the Faulhorn, have gone far to confirm it. Having fed themselves, before and during the ascent, upon starch, fat, and sugar, avoiding all nitrogenised compounds, they found that the consumption of such food was amply sufficient to supply the force necessary for their expedition, and that they felt no exhaustion. By appropriate chemical examination they ascertained that there was no notable increase in the oxidation of the nitrogenised constituents of the body. After calculating the mechanical equivalents of the combustion effected, they state, as their first conclusion, that 'the burning of protein substances cannot be the only source of muscular power, for we have here two cases in which men performed

more measurable work than the equivalent of the amount of heat which, taken at a most absurdly high figure, could be calculated to result from the burning of the albumen.'

They further go on to state that, so far from the oxidation of albuminous substances being the only source of muscular power, 'the substances by the burning of which force is generated in the muscles are not the albuminous constituents of those tissues, but non-nitrogenous substances, either fats or hydrates of carbon,' and that the burning of albumen is not in any way concerned in the production of muscular power.

We must not confuse the question of the food which forms and repairs muscle and gives permanent capability of muscular force with that which supplies the requisites for temporary activity; no doubt the carnivora are the most powerfully constituted animals, but the chamois, gazelle, &c., have great temporary capacity for muscular exertion, though their food is vegetable; for concentrated and sustained energy, however, they do not equal the carnivora; and with the domestic graminivora we certainly find that they are capable of performing more continuous work when supplied with those vegetables which contain the greatest quantity of nitrogen.

These and many similar classes of research show that in chemical enquiries, as in other branches of

science, we are gradually relieving ourselves of hypothetical existences, which certainly had the advantage that they might be varied to suit the requirements of the theorist.

Phlogiston, as Lavoisier said with a sneer, was sometimes heavy, sometimes light; sometimes fire in a free state, sometimes combined; sometimes passing through glass vessels, sometimes retained by them; which by its protean changes explained causticity and non-causticity, transparency and opacity, colours and their absence. As phlogiston and similar creations of the mind have passed away, so with hypothetic fluids, imponderable matters, specific ethers, and other inventions of entities made to vary according to the requirements of the theorist, I believe the day is approaching when these will be dispensed with, and when the two fundamental conceptions of matter and motion will be found sufficient to explain physical phenomena.

The facts made known to us by geological enquiries, while on the one hand they afford striking evidence of continuity, on the other, by the breaks in the record, may be used as arguments against it. The great question once was, whether these chasms represent sudden changes in the formation of the earth's crust, or whether they arise from dislocations occasioned since the original deposition of strata or from gradual shifting of the areas

of submergence. Few geologists of the present day would, I imagine, not adopt the latter alternatives. Then comes a second question, whether, when the geological formation is of a continuous character, the different characters of the fossils represent absolutely permanent varieties, or may be explained by gradual modifying changes.

Professor Ansted, summing up the evidence on this head as applied to one division of stratified rocks, writes as follows:—'Palæontologists have endeavoured to separate the Lias into a number of subdivisions, by the Ammonites, groups of species of those shells being characteristic of different zones. The evidence on this point rests on the assumption of specific differences being indicated by permanent modifications of the structure of the shell. But it is quite possible that these may mean nothing more than would be due to some change in the conditions of existence. Except between the Marlstone and the Upper Lias there is really no palæontological break, in the proper sense of the words; alterations of form and size consequent on the occurrence of circumstances more or less favourable, migration of species, and other well-known causes, sufficiently account for many of those modifications of the form of the shell that have been taken as specific marks. This view is strengthened by the fact that other shells and other organisms generally show no proof of a

break of any importance except at the point already alluded to.'

But, irrespectively of another deficiency in the geological record, which will be noticed presently, the physical breaks in the stratification make it next to impossible to fairly trace the order of succession of organisms by the evidence afforded by their fossil remains. Thus there are nine great breaks in the Palæozoic series, four in the Secondary, and one in the Tertiary, besides those between Palæozoic and Secondary and Secondary and Tertiary respectively. Thus in England there are sixteen important breaks in the succession of strata, together with a number of less important interruptions. But although these breaks exist, we find pervading the works of many geologists a belief, resulting from the evidence presented to their minds, sometimes avowed, sometimes unconsciously implied, that the succession of species bears some definite relation to the succession of strata. Thus Professor Ramsay says, that 'In cases of superposition of fossiliferous strata, in proportion as the species are more or less continuous, that is to say, as the break in the succession of life is partial or complete, so was the time that elapsed between the close of the lower and the commencement of the upper strata a shorter or a longer interval. The break in life may be indicated not only by a difference in species, but yet more importantly by

the absence of older and appearance of newer allied or unallied genera.'

Indications of the connection between cosmical studies and geological researches are dawning on us: there is, for instance, some reason to believe that we can trace many geological phenomena to our varying rotation round the sun; thus more than thirty years ago Sir J. Herschel proposed an explanation of the changes of climate on the earth's surface as evidenced by geological phenomena, founded on the changes of eccentricity in the earth's orbit.

He said he had entered on the subject 'impressed with the magnificence of that view of geological revolutions which regards them rather as regular and necessary efforts of great and general causes, than as resulting from a series of convulsions and catastrophes regulated by no laws and reducible to no fixed principles.'

As the mean distance of the earth from the sun is nearly invariable, it would seem at first sight that the mean annual supply of light and heat received by the earth would also be invariable; but according to his calculations it is inversely proportional to the minor axis of the orbit: this would give less heat when the eccentricity of the earth's orbit is approaching towards or at its minimum. Mr. Croll has recently shown reason to believe that the climate, at all events in the

circumpolar and temperate zones of the earth, would depend on whether the winter of a given region occurred when the earth at its period of greatest eccentricity was in aphelion or perihelion —if the former, the annual average of temperature would be lower, if the latter, it would be higher than when the eccentricity of the earth's orbit were less or approached more nearly to a circle. He calculates the difference in the amount of heat at the period of maximum eccentricity of the earth's orbit to be as 19 to 26, according as the winter would take place when the earth was in aphelion or in perihelion. His reason may be briefly stated thus: assuming the mean annual heat to be the same, whatever the eccentricity of orbit, yet if the extremes of heat and cold in winter and summer be greater, a colder climate will prevail, for there will be more snow and ice accumulated in the cold winter than the hot summer can melt, a result aided by the shelter from the sun's rays produced by the vapour suspended in consequence of the aqueous evaporation; hence we should get glacial periods, when the orbit of the earth is at its greatest eccentricity, at those parts of the earth's surface where it is winter when the earth is in aphelion; carboniferous or hot periods where it is winter in perihelion; and normal or temperate periods when the eccentricity of orbit is at a minimum; all these would gradually slide into

each other, and would produce at long distant periods alternations of cold and heat, several of which we actually observe in geological records.

If this theory be borne out, we should approximate to a test of the time which has elapsed between different geological epochs. Mr. Croll's computation of this would make it certainly not less than 100,000 years since the last glacial epoch, a time not very long in geological chronology—probably it is much more.

When we compare with the old theories of the earth, by which the apparent changes on its surface were accounted for by convulsions and cataclysms, the modern view inaugurated by Lyell, your former President, and now, if not wholly, at all events to a great extent adopted, it seems strange that the referring past changes to similar causes to those which are now in operation should have remained uninvestigated until the present century; but with this, as with other branches of knowledge, the most simple is frequently the latest view which occurs to the mind. It is much more easy to invent a *Deus ex machinâ* than to trace out the influence of slow continuous change; the love of the marvellous is so much more attractive than the patient investigation of truth, that we find it to have prevailed almost universally in the early stages of science.

In astronomy we had crystal spheres, cycles,

and epicycles; in chemistry the philosopher's stone, the elixir vitæ, the archæus or stomach demon, and phlogiston; in electricity the notion that amber possessed a soul, and that a mysterious fluid could knock down a steeple. In geology a deluge or a volcano was supplied. In palæontology a new race was created whenever theory required it: how such new races began, the theorist did not stop to enquire.

A curious speculator might say to a palæontologist of even recent date, in the words of Lucretius,

> 'Nam neque de cœlo cecidisse animalia possunt
> Nec terrestria de salsis exisse lacunis.
>
> * * * * *
>
> E nihilo si crescere possent,
> (Tum) fierent juvenes subito ex infantibus parvis,
> E terrâque exorta repente arbusta salirent;
> Quorum nil fieri manifestum est, omnia quando
> Paulatim crescunt, ut par est, semine certo,
> Crescentesque genus servant'

—which may be thus freely paraphrased: 'You have abandoned the belief in one primæval creation at one point of time, you cannot assert that an elephant existed when the first saurians roamed over earth and water. Without, then, in any way limiting Almighty power, if an elephant were created without progenitors, the first elephant must, in some way or other, have physically arrived on this earth. Whence did he come? did he fall from

the sky (*i.e.* from interplanetary space)? did he rise moulded out of a mass of amorphous earth or rock? did he appear out of the cleft of a tree? If he had no antecedent progenitors, some such beginning must be assigned to him.' I know of no scientific writer who has, since the discoveries of geology have become familiar, ventured to present in intelligible terms any definite notion of how such an event could have occurred: those who do not adopt some view of continuity are content to say God willed it; but would it not be more reverent and more philosophical to enquire by observation and experiment, and to reason from induction and analogy, as to the probabilities of such frequent miraculous interventions?

I know I am touching on delicate ground, and that a long time may elapse before that calm enquiry after truth which it is the object of associations like this to promote can be fully attained; but I trust that the members of this body are sufficiently free from prejudice, whatever their opinions may be, to admit an enquiry into the general question whether what we term species are and have been rigidly limited, and have at numerous periods been created complete and unchangeable, or whether, in some mode or other, they have not gradually and indefinitely varied, and whether the changes due to the influence of surrounding circumstances, to efforts to accommodate themselves to

surrounding changes, to what is called natural selection, or to the necessity of yielding to superior force in the struggle for existence, as maintained by our illustrious countryman Darwin, and other causes, have not so modified organisms as to enable them to exist under changed conditions. I am not going to put forward any theory of my own, I am not going to argue in support of any special theory, but having endeavoured to show how, as science advances, the continuity of natural phenomena becomes more apparent, it would be cowardice not to present some of the main arguments for and against continuity as applied to the history of organic beings.

As we detect no such phenomenon as the creation or spontaneous generation of vegetables and animals which are large enough for the eye to see without instrumental assistance, as we have long ceased to expect to find a Plesiosaurus spontaneously generated in our fish-pond, or a Pterodactyle in our pheasant-cover, the field of this class of research has become identified with the field of the microscope, and at each new phase the investigation has passed from a larger to a smaller class of organisms. The question whether among the smallest and apparently the most elementary forms of organic life the phenomenon of spontaneous generation obtains, has recently formed the subject of careful experiment and animated discussion in France. If it could be found that organisms of a

complex character were generated without progenitors out of amorphous matter, it might reasonably be argued that a similar mode of creation might obtain in regard to larger organisms. Although we see no such phenomenon as the formation of an animal such as an elephant, or a tree such as an oak, excepting from a parent which resembles it, yet if the microscope revealed to us organisms, smaller but equally complex, so formed without having been reproduced, it would render it not improbable that such might have been the case with larger organic beings. The controversy between M. Pasteur and M. Pouchet has led to a very close investigation of this subject, and the general opinion is that when such precautions are taken as exclude from the substance submitted to experiment all possibility of germs from the atmosphere being introduced, as by passing the air which is to support the life of the animalculæ through tubes heated to redness and other precautions, no formation of organisms takes place. Some experiments of Mr. Child's, communicated to the Royal Society during the last year, again throw doubt on the negative results obtained by M. Pasteur; so that the question may be not finally determined, but the balance of experiment and opinion is against spontaneous generation.

One argument presented by M. Pasteur is well worthy of remark, viz. that in proportion as our

means of scrutiny become more searching, heterogeny, or the developement of organisms without generation from parents of similar organism, has been gradually driven from higher to lower forms of life, so that if some apparent exceptions still exist they are of the lowest and simplest forms, and these exceptions may probably be removed, as M. Pasteur considers he has removed them, by a more searching investigation.

If it be otherwise, if heterogeny obtains at all, few will not now admit that at present the result of the most careful experiments shows it to be confined to the more simple organic structures, and that all the progressive and more highly developed forms are, as far as the most enlarged experience shows, generated by reproduction.

The great difficulty which is met with at the threshold of enquiry into the origin of species is the definition of species; in fact, species can hardly be defined without begging the question in dispute.

Thus if species be said to be a perseverance of type incapable of blending itself with other types, or, which comes nearly to the same thing, incapable of producing by union with other types offspring of an intermediate character which can again reproduce, we arrive at this result, that whenever the advocate of continuity shows a blending of

what had been hitherto deemed separate species, the answer is, they were considered separate species by mistake, they do not now come under the definition of species, because they interbreed.

The line of demarcation is thus *ex hypothesi* removed a step further, so that unless the advocate of continuity can, on his side, prove the whole question in dispute, by showing that all can directly or by intermediate varieties reproduce, he is defeated by the definition itself of species.

On the other hand, if this, or something in fact amounting to it, be not the definition of species—if it be admitted that distinct species can, under certain favourable conditions, produce intermediate offspring capable of reproduction, then continuity in some mode or other is admitted.

The question then takes this form. Are there species or are there not? Is the word to be used as signifying a real, natural distinction, or as a mere convenient designation applied to subdivisions having a permanence which will probably outlive man's discussions on the subject, but not an absolute fixity? The same question, in a wider sense, and taking into consideration a much longer time, would be applicable to genera and families.

Actual experiment has done little to elucidate the question, nor, unless we can suppose the experiments continued through countless generations, is it likely to contribute much to its solution. We

must therefore have recourse to the enlarged experience or induction from the facts of geology, palæontology, and physiology, aided by analogy from the laws of action which nature evidences in other departments.

The doctrine of gradual succession is hardly yet formularised, and though there are some high authorities for certain modifications of such view, the preponderance of authority would necessarily be on the other side. Geology and palæontology are recent sciences, and we cannot tell what the older authors would have thought or written had the more recently discovered facts been presented to their view. Authority, therefore, does not much help us on this question.

Geological discoveries seemed, in the early period of the science, to show complete extinction of certain species and the appearance of new ones, great gaps existing between the characteristics of the extinct and the new species. As science advanced, these were more or less filled up, and the difficulty in the first instance of admitting unlimited modification of species would seem to have arisen from the comparison of the extreme ends of the scale where the intermediate links or some of them were wanting.

To suppose a Zoophyte the progenitor of a Mammal, or to suppose at some particular period of time a highly developed animal to have come

out of nothing, or suddenly grown out of inorganic matter, would appear at first sight equally extravagant hypotheses. As an effort of Almighty creative power, neither of these alternatives presents more difficulty than the other; but as we have no means of ascertaining how creative power worked, but by an examination and study of the works themselves, we are not likely to get either view proved to ocular demonstration. A single phase in the progress of natural transmutation would probably require a term far transcending all that embraced by historical records; and on the other hand, it might be said, sudden creations, though taking place frequently, if viewed with reference to the immensity of time involved in geological periods, may be so rare with reference to our experience, and so difficult of clear authentication, that the non-observation of such instances cannot be regarded as absolute disproof of their possible occurrence.

The more the gaps between species are filled up by the discovery of intermediate varieties the stronger becomes the argument for transmutation and the weaker that for successive creations, because the former view then becomes more and more consistent with experience, the latter more discordant from it. As undoubted cases of variation, more or less permanent, from given characteristics, are produced by the effects of climate, food,

domestication, &c., the more species are increased by intercalation, the more the distinctions slide down towards those which are within the limits of such observed deviations; while on the other hand, to suppose the more and more frequent recurrence of fresh creations out of amorphous matter is a multiplication of miracles or special interventions not in accordance with what we see of the uniform and gradual progress of nature, either in the organic or inorganic world. If we were entitled to conclude that the progress of discovery would continue in the same course, and that species would become indefinitely multiplied, the distinctions would become infinitely minute, and all lines of demarcation would cease, the polygon would become a circle, the succession of points a line. Certain it is that the more we observe the more we increase the subdivision of species, and consequently the number of these supposed creations; so that new creations become innumerable, and yet of these we have no one well-authenticated instance, and in no other observed operation of nature have we seen this want of continuity, these frequent *per saltum* deviations from uniformity, each of which is a miracle.

The difficulty of producing intermediate offspring from what are termed distinct species and the infecundity in many instances of hybrids are used as strong arguments against continuity of succession;

on the other hand, it may be said long-continued variation through countless generations has given rise to such differences of physical character that reproduction is difficult in some cases, and in others impossible.

Suppose, for instance, M to represent a parent-race whose offspring by successive changes through eons of time have divaricated, and produced on the one hand a species A, and on the other a species Z, the changes here have been so great that we should never expect directly to reproduce an intermediate between A and Z. A and B on the one hand, and Y and Z on the other, might reproduce; but to regain the original type M, we must not only retrocede through all the intermediates, but must have similar circumstances recalled in an inverse order at each phase of retrogression, conditions which it is obviously impossible to fulfil. But though among the higher forms of organic structure we cannot retrace the effects of time and reproduce intermediate types, yet among some of the lower forms we find it difficult to assign any line of specific demarcation; thus as a result of the very elaborate and careful investigations of Dr. Carpenter on Foraminifera, he states, 'It has been shown that a very wide range of variation exists among Orbitolites, not merely as regards external form, but also as to plan of developement; and not merely as to the shape and aspect of the entire

organism, but also with respect to the size and configuration of its component parts. It would have been easy, by selecting only the most divergent types from amongst the whole series of specimens which I have examined, to prefer an apparently substantial claim on behalf of these to be accounted as so many distinct species. But after having classified the specimens which could be arranged around these types, a large proportion would yet have remained, either presenting characters intermediate between those of two or more of them, or actually combining those characters in different parts of their fabric; thus showing that no lines of demarcation can be drawn across any part of the series that shall definitely separate it into any number of groups, each characterised by features entirely peculiar to itself.'

At the conclusion of his enquiry he states—

I. The range of variation is so great among Foraminifera as to include not merely the differential characters which systematists proceeding upon the ordinary methods have accounted specific, but also those upon which the greater part of the genera of this group have been founded, and even in some instances those of its orders.

II. The ordinary notion of species as assemblages of individuals marked out from each other by definite characters that have been genetically transmitted from original proto-types similarly distin-

guished, is quite inapplicable to this group; since even if the limits of such assemblages were extended so as to include what elsewhere would be accounted genera, they would still be found so intimately connected by gradational links that definite lines could not be drawn between them.

III. The only natural classification of the vast aggregate of diversified forms which this group contains will be one which ranges them according to their direction and degree of divergence from a small number of principal family types; and any subordinate grouping of genera and species which may be adopted for the convenience of description and nomenclature must be regarded merely as assemblages of forms characterised by the nature and degree of the modifications of the original type, which they may have respectively acquired in the course of genetic descent from a common ancestry.

IV. Even in regard to these family types it may fairly be questioned whether analogical evidence does not rather favour the idea of their derivation from a common original than that of their primitive distinctness.

Mr. H. Bates, when investigating 'The Lepidoptera of the Amazon Valley,' may almost be said to have witnessed the origin of some species of Butterflies, so close have been his observations on the habits of these animals that have led to their variation and segregation, so closely do the results

follow his observations, and so great is the difficulty of otherwise accounting for any of the observed facts.

In the numerous localities of the Amazon region certain gregarious species of Butterfly (*Heliconidea*) swarm in incredible numbers, almost outnumbering all the other butterflies in the neighbourhood; the species in the different localities being different, though often to be distinguished by a very slight shade.

In these swarms are to be found, in small numbers, other species of butterflies belonging to as many as ten different genera, and even some moths; and these intruders, though they structurally differ *in toto* from the swarms they mingle with, and from one another, mimic the *Heliconideæ* so closely in colours, habits, mode of flight, &c., that it is almost impossible to distinguish the intruders from those they mingle with. The obvious benefit of this mimicry is safety, the intruders hence escaping detection by predatory animals.

Mr. Bates has extended his observations to the habits of life, food, variations and geographical range of the species concerned in these mimetic phenomena, and finds in every case corroborative evidence of every variety and species being derivative, the species being modified from place to place to suit the peculiar form of *Heliconidea* stationed there.

Mr. Wallace has done similar service to the derivative theory by his observations and writings on the Butterflies and Birds of the Malay Archipelago, adducing instances of mimetic resemblances strictly analogous to the above; and adding in further illustration a beautiful series of instances where the form of the wing of the same butterfly is so modified in various islets as to produce changes in their mode of flight that tend to the conservation of the variety by aiding its escape when chased by birds or predacious insects.

He has also adduced a multitude of examples of geographical and representative species, races, and varieties, forming so graduated a series as to render it obvious that they have had a common origin.

The effect of food in the formation and segregation of races and of certain groups of insects has been admirably demonstrated by Mr. B. D. Walsh, of North America.

Mr. McDonnell has been led to the discovery of a new organ in electric fishes from the application of the theory of descent, and Dr. Fritz Müller has published numerous observations showing that organs of very different structure may, through the operation of natural selection, acquire very similar and even identical functions. Sir John Lubbock's diving hymenopterous insect affords a remarkable illustration of analogous phenomena; it dives by

the aid of its wings, and is the only insect of the vast order it belongs to that is at all aquatic.

The discovery of the Eozoon is of the highest importance in reference to the derivative hypothesis, occurring as it does in strata that were formed at a period inconceivably antecedent to the presupposed introduction of life upon the globe, and displacing the argument derived from the supposition that at the dawn of life a multitude of beings of high organisation were simultaneously developed (in the Silurian and Cambrian strata).

Professor A. De Candolle, one of the most distinguished continental botanists, has, to some extent, abandoned the tenets held in his 'Géographie Botanique,' and favours the derivative hypothesis in his paper on the variation of oaks; following up a paper, by Dr. Hooker, on the oaks of Palestine, showing that some sixteen of them are derivative, he avows his belief that two-thirds of the 300 species of this genus, which he himself describes, are provisional only.

Dr. Hooker, who had only partially accepted the derivative hypothesis propounded before the publication of 'The Origin of Species through Natural Selection,' at the same time declining the doctrine of special creation, has since then cordially adopted the former, and illustrated its principles by applying them to the solution of various botanical questions: first, in reference to the flora of Australia,

the anomalies of which he appears to explain satisfactorily by the application of these principles; and, latterly, in reference to the Arctic flora.

In the case of the Arctic flora, he believes that originally Scandinavian types were spread over the high northern latitudes; that these were driven southwards during the glacial period, when many of them changed their forms in the struggle that ensued with the displaced temperate plants; that on the returning warmth, the Scandinavian plants, whether changed or not, were driven again northwards and up to the mountains of the temperate latitudes, followed in both cases by series of pre-existing plants of the temperate Alps. The result is the present mixed Arctic flora, consisting of a basis of more or less changed and unchanged Scandinavian plants, associated in each longitude with representatives of the mountain flora of the more temperate regions to the south of them.

The publication of a previously totally unknown flora, that of the Alps of tropical Africa, by Dr. Hooker, has afforded a multitude of facts that have been applied in confirmation of the derivative hypothesis. This flora is found to have relationships with those of temperate Europe and North Africa, of the Cape of Good Hope, and of the mountains of tropical Madagascar and Abyssinia, that can be accounted for on no other hypothesis but that there has been ancient climatal connexion

and some coincident or subsequent slight changes of specific character.

The doctrine of Cuvier, every day more and more borne out by observation, that each organ bears a definite relation to the whole of the individual, seems to support the view of indefinite variation. If an animal seeks its food or safety by climbing trees, its claws will become more prehensile, the muscles which act upon those claws must become more developed, the body will become agile by the very exercise which is necessary to it, and each portion of the frame will mould itself to the wants of the animal by the effect on it of the habits of the animal.

Another series of facts which present an argument in favour of gradual succession are the phases of resemblance to inferior orders which the embryo passes through in its developement, and the relations shown in what is termed the metamorphosis of plants; facts difficult to account for on the theory of frequent separate creations, but almost inevitable on that of gradual succession. So also, the existence of rudimentary and effete organs, which must either be referred to a *lusus naturæ* or to some mode of continuous succession.

The doctrine of typical nuclei seems only a mode of evading the difficulty; experience does not give us the types of theory, and, after all, what are these types? It must be admitted there

are none such in reality; how are we led to the theory of them? simply by a process of abstraction from classified existences. Having grouped from natural similitudes certain forms into a class, we select attributes common to each member of the class, and call the assemblage of such attributes a type of the class. This process gives us an abstract idea, and we then transfer this idea to the Creator, and make Him start with that which our own imperfect generalisation has derived. It seems to me that the doctrine of types is, in fact, a concession to the theory of continuity or indefinite variability; for the admission that large groups have common characters shows, necessarily, a blending of forms within the scope of the group, which supports the view of each member being derived from some other member of it: can it be asserted that the assigned limits of such groups have a definite line of demarcation?

The condition of the earth's surface, or at least of large portions of it, has for long periods remained substantially the same; this would involve a greater degree of fixity in the organisms which have existed during such periods of little change than in those which have come into being during periods of more rapid transition; for, though rejecting catastrophes as the general *modus agendi* of nature, I am far from saying that the march of physical changes has been always perfectly uniform.

There have been doubtless what may be termed secular seasons, and there have been local changes of varying degrees of extent and permanence; from such causes organised beings would be more concentrated in certain directions than in others, the fixity of character being in the ratio of the fixity of condition. This would throw natural forms into certain groups which would be more prominent than others, like the colours of the rainbow, which present certain predominant tints though they merge into each other by insensible gradations.

While the evidence seems daily becoming stronger in favour of a derivative hypothesis as applied to the succession of organic beings, we are far removed from anything like a sufficient number of facts to show that, at all events within the existing geological periods capable of being investigated, there has been any great progression from a simpler or more embryonic to a more complex type.

Professor Huxley, though inclined to the derivative hypothesis, shows, in the concluding portion of his address to the Geological Society, 1862, a great number of cases in which, though there is abundant evidence of variation, there is none of progression. There are, however, several groups of Vertebrata in which the endoskeleton of the older presents a less ossified condition than that of the younger genera. He cites the Devonian

Ganoids, the Mesozoic Lepidosteidæ, the Palæozoic Sharks, and the more ancient Crocodilia and Lacertilia, and particularly the Pycnodonts and Labyrinthodonts, as instances of this when compared with their more recent representatives.

The records of life on the globe may have been destroyed by the fusion of the rocks, which would otherwise have preserved them, or by crystallisation after hydrothermal action. The earlier forms may have existed at a period when this planet was in course of formation, or being segregated or detached from other worlds or systems. We have not evidence enough to speculate on the subject, but by time and patience we may acquire it.

Were all the forms which have existed embalmed in rock the question would be solved; but what a small proportion of extinct forms is so preserved, and must be, if we consider the circumstances necessary to fossilise organic remains. On the dry land, unwashed by rivers and seas, when an animal or plant dies, it undergoes chemical decomposition which changes its form; it is consumed by insects, its skeleton is oxidised and crumbles into dust. Of the myriads of animals and vegetables which annually perish we find hardly an instance of a relic so preserved as to be likely to become a permanent fossil. So again in the deeper parts of the ocean, or of the larger lakes, the few fish there are perish and their remains sink to the

bottom, and are there frequently consumed by other marine or lacustrine organisms or chemically decomposed. As a general rule, it is only when the remains are silted up by marine, fluviatile or lacustrine sediments that the remains are preserved. Geology, therefore, might be expected to keep for us mainly such organic remains as inhabited deltas or the margins of seas, lakes, or rivers; here and there an exception may occur, but the mass of preserved relics would be those of creatures so situated; and so we find it, the bulk of fossil remains consists of fish and amphibia, shell-fish form the major part of the geological museum, limestone and chalk rocks frequently consisting of little else than a congeries of fossil shells. Plants of reed or rush-like character, fish which are capable of inhabiting shallow waters, and saurian animals, form another large portion of geological remains.

Compare the shell-fish and amphibia of existing organisms with the other forms, and what a small proportion they supply; compare the shell-fish and amphibia of Palæontology with the other forms, and what an overwhelming majority they yield.

There is nothing, as Professor Huxley has remarked, like an extinct order of birds or mammals, only a few isolated instances. It may be said the ancient world possessed a larger proportion of fish and amphibia, and was more suited to their existence. I see no reason for believing this, at

least to anything like the extent contended for; the fauna and flora now in course of being preserved for future ages would give the same idea to our successors.

Crowded as Europe is with cattle, birds, insects, &c., how few are geologically preserved! while the muddy or sandy margins of the ocean, the estuaries, and deltas are yearly accumulating numerous crustacea and mollusca, with some fishes and reptiles, for the study of future palæontologists.

If this position be right, then, notwithstanding the immense number of preserved fossils, there must have lived an immeasurably larger number of unpreserved organic beings, so that the chance of filling up the missing links, except in occasional instances, is very slight. Yet where circumstances have remained suitable for their preservation, many closely connected species are preserved—in other words, while the intermediate types in certain cases are lost, in others they exist. The opponents of continuity lay all stress on the lost and none on the existing links.

But there is another difficulty in the way of tracing a given organism to its parent forms, which, from our conventional mode of deducing genealogies, is never looked upon in its proper light.

Where are we to look for the remote ancestor of a given form? Each of us, supposing none of our

progenitors to have intermarried with relatives, would have had at or about the period of the Norman Conquest upwards of a hundred million direct ancestors of that generation, and if we add the intermediate ancestors, double that number. As each individual has a male and female parent, we have only to multiply by two for each thirty years, the average duration of a generation, and it will give the above result.

Let any one assume that one of his ancestors at the time of the Norman Conquest was a Moor, another a Celt, and a third a Laplander, and that these three were preserved while all the others were lost, he would never recognise either of them as his ancestor, he would only have the one-hundred millionth of the blood of each of them, and as far as they were concerned there would be no perceptible sign of identity of race.

But the problem is more complex than that which I have stated; at the time of the Conquest there were hardly a hundred million people in Europe, it follows that a great number of the ancestors of the *propositus* must have intermarried with relations, and then the pedigree, going back to the time of the Conquest, instead of being represented by diverging lines, would form a network so tangled that no skill could unravel it; the law of probabilities would indicate that any two people in the same country, taken at hazard, would not

have many generations to go back before they would find a common ancestor, who probably, could they have seen him or her in the life, had no traceable resemblance to either of them. Thus two animals of a very different form, and of what would be termed very different species, might have a common geological ancestor, and yet the skill of no comparative anatomist could trace the descent.

From the long-continued conventional habit of tracing pedigrees through the male ancestor, we forget in talking of progenitors that each individual has a mother as well as a father, and there is no reason to suppose that he has in him less of the blood of the one than of the other.

The recent discoveries in palæontology show us that Man existed on this planet at an epoch far anterior to that commonly assigned to him. The instruments connected with human remains, and indisputably the work of human hands, show that to these remote periods the term civilisation could hardly be applied—chipped flints of the rudest construction, probably, in the earlier cases, fabricated by holding an amorphous flint in the hand and chipping off portions of it by striking it against a larger stone or rock; then, as time suggested improvements, it would be more carefully shaped, and another stone used as a tool; then (at what interval we can hardly guess) it would be ground, then roughly polished, and so on—subse-

quently bronze weapons, and nearly the last before we come to historical periods, iron. Such an apparently simple invention as a wheel must, in all probability, have been far subsequent to the rude hunting-tools or weapons of war to which I have alluded.

A little step-by-step reasoning will convince the unprejudiced that what we call civilisation must have been a gradual process; can it be supposed that the inhabitants of Central America or of Egypt suddenly and what is called instinctively built their cities, carved and ornamented their monuments? If not, if they must have learned to construct such erections, did it not take time to acquire such learning, to invent tools as occasion required, contrivances to raise weights, rules or laws by which men acted in concert to effect the design? Did not all this require time? and if, as the evidence of historical times shows, invention marches with a geometrical progression, how slow must have been the earlier steps! If even now habit, and prejudice resulting therefrom, vested interests, &c., retard for some time the general application of a new invention, what must have been the degree of retardation among the comparatively uneducated beings which then existed?

I have of course been able to indicate only a few of the broad arguments on this most interesting subject; for detailed results the works of Darwin,

Hooker, Huxley, Carpenter, Lyell, and others must be examined. If I appear to lean to the view that the successive changes in organic beings do not take place by sudden leaps, it is, I believe, from no want of an impartial feeling; but if the facts are stronger in favour of one theory than another, it would be an affectation of impartiality to make the balance appear equipoised.

The prejudices of education and associations with the past are against this as against all new views; and while on the one hand a theory is not to be accepted because it is new and *primâ facie* plausible, still to this assembly I need not say that its running counter to existing opinions is not necessarily a reason for its rejection; the *onus probandi* should rest on those who advance a new view, but the degree of proof must differ with the nature of the subject. The fair question is, Does the newly-proposed view remove more difficulties, require fewer assumptions, and present more consistency with observed facts than that which it seeks to supersede? If so, the philosopher will adopt it, and the world will follow the philosopher—after many days.

It must be borne in mind that even if we are satisfied from a persevering and impartial enquiry that organic forms have varied indefinitely in time, the *causa causans* of these changes is not explained by our researches; if it be admitted that we find

no evidence of amorphous matter suddenly changed into complex structure, still why matter should be endowed with the plasticity by which it slowly acquires modified structure is unexplained. If we assume that natural selection, or the struggle for existence, coupled with the tendency of like to reproduce like, gives rise to various organic changes, still our researches are at present uninstructive as to why like should produce like, why acquired characteristics in the parent should be reproduced in the offspring. Reproduction itself is still an enigma, and this great question may involve deeper thoughts than it would be suitable to enter upon now.

Perhaps the most convincing argument in favour of continuity which could be presented to a doubting mind would be the difficulty it would feel in representing to itself any *per saltum* act of nature. Who would not be astonished at beholding an oak tree spring up in a day, and not from seed or shoot? We are forced by experience, though often unconsciously, to believe in continuity as to all effects now taking place; if any one of them be anomalous we endeavour, by tracing its history and concomitant circumstances, to find its cause, *i.e.* to relate it to antecedent phenomena; are we then to reject similar enquiries as to the past? is it laudable to seek an explanation of present changes by observation, experiment, and analogy, and yet reprehen-

sible to apply the same mode of investigation to the past history of the earth and of the organic remains embalmed in it?

If we disbelieve in sudden creations of matter or force, in the sudden formations of complex organisms now, if we now assign to the heat of the sun an action enabling vegetables to live by assimilating gases and amorphous earths into growing structures, why should such effects not have taken place in earlier periods of the world's history, when the sun shone as now, and when the same materials existed for his rays to fall upon?

If we are satisfied that continuity is a law of nature, the true expression of the action of Almighty Power, then, though we may humbly confess our inability to explain why matter is impressed with this tendency to gradual structural formation, we should cease to look for special interventions of creative power in changes which are difficult to understand, because, being removed from us in time, their concomitants are lost; we should endeavour from the relics to evoke their history, and when we find a gap not try to bridge it over with a miracle.

If it be true that continuity pervades all physical phenomena, the doctrine applied by Cuvier to the relations of the different parts of an animal to each other might be capable of great extension. All the phenomena of inorganic and organised matter

might be expected to be so inter-related that the study of an isolated phenomenon would lead to a knowledge of numerous other phenomena with which it is connected. As the antiquary deduces from a monolith the tools, the arts, the habits, and epoch of those by whom it is wrought, so the student of science may deduce from a spark of electricity or ray of light the source whence it is generated; and by similar processes of reasoning other phenomena hitherto unknown may be deduced from their probable relation with the known. But, as with heat, light, magnetism, and electricity, though we may study the phenomena to which these names have been given, and their mutual relations, we know nothing of what they are; so, whether we adopt the view of natural selection, of effort, of plasticity, &c., we know not why organisms should have this *nisus formativus*, or why the acquired habit or exceptional quality of the individual should reappear in the offspring.

Philosophy ought to have no likes or dislikes, truth is her only aim; but if a glow of admiration be permitted to a physical enquirer, to my mind a far more exquisite sense of the beautiful is conveyed by the orderly developement, by the necessary inter-relation and inter-action of each element of the cosmos, and by the conviction that a bullet falling to the ground changes the dynamical conditions of

the universe, than can be conveyed by mysteries, by convulsions, or by cataclysms.

The sense of understanding is to the educated more gratifying than the love of the marvellous, though the latter need never be wanting to the nature-seeker.

But the doctrine of continuity is not solely applicable to physical enquiries.

The same modes of thought which lead us to see continuity in the field of the microscope as in the universe, in infinity downwards as in infinity upwards, will lead us to see it in the history of our own race; the revolutionary ideas of the so-called natural rights of man, and *à priori* reasoning from what are termed first principles, are far more unsound and give us far less ground for improvement of the race than the study of the gradual progressive changes arising from changed circumstances, changed wants, changed habits. Our language, our social institutions, our laws, the constitution of which we are proud, are the growth of time, the product of slow adaptations, resulting from continuous struggles. Happily in this country practical experience has taught us to improve rather than to remodel; we follow the law of nature and avoid cataclysms.

The superiority of Man over other animals inhabiting this planet, of civilised over savage man, and of the more civilised over the less civilised, is

proportioned to the extent which his thought can grasp of the past and of the future. His memory reaches further back, his capability of prediction reaches further forward in proportion as his knowledge increases. He has not only personal memory which brings to his mind at will the events of his individual life—he has history, the memory of the race; he has geology, the history of the planet; he has astronomy, the geology of other worlds. Whence does the conviction to which I have alluded, that each material form bears in itself the records of its past history, arise? Is it not from the belief in continuity? Does not the worn hollow on the rock record the action of the tide, its stratified layers the slow deposition by which it was formed, the organic remains imbedded in it the beings living at the times these layers were deposited, so that from a fragment of stone we can get the history of a period myriads of years ago? From a fragment of bronze we may get the history of our race at a period antecedent to tradition. As science advances our power of reading such history improves and is extended. Saturn's ring may help us to a knowledge of how our solar system developed itself, for it as surely contains that history as the rock contains the record of its own formation.

By this patient investigation how much have we already learned, which the most civilised of ancient

human races ignored! While in ethics, in politics, in poetry, in sculpture, in painting, we have scarcely, if at all, advanced beyond the highest intellects of ancient Greece or Italy, how great are the steps we have made in physical science and its applications!

But how much more may we not expect to know?

We, this evening assembled, Ephemera as we are, have learned by transmitted labour, to weigh, as in a balance, other worlds larger and heavier than our own, to know the length of their days and years, to measure their enormous distance from us and from each other, to detect and accurately ascertain the influence they have on the movements of our world and on each other, and to discover the substances of which they are composed; may we not fairly hope that similar methods of research to those which have taught us so much may give our race further information, until problems relating not only to remote worlds, but possibly to organic and sentient beings which may inhabit them, problems which it might now seem wildly visionary to enunciate, may be solved by progressive improvements in the modes of applying observation and experiment, induction and deduction.

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